plants, which cannot be grown under similar conditions. The fact that they will grow freely in soil containing ammonia, or decomposing animal matter convertible into ammonia, led to the conclusion that they wanted nitrogenous food. The fact that the nitrogen is not an important element of their substance at any period leads me to infer that these plants are incapable of decomposing water, and consequently dependent for their necessary supply of hydrogen upon ammonia or some other compound of hydrogen more readily decomposed than water. It is well known that while the nitrates of potash, soda, lime, etc., are all valuable auxiliaries to farmyard manures, they are of no value as a substitute for it. Very eminent chemists have been somewhat staggered at the results of their experiments in this direction; but precisely as the function of nitrogen in ammonia is to carry hydrogen, so the function of the nitrogen in the nitrates is to carry potash. Whether we dress the soil with nitrate of soda, lime, or potash, the result is the same. With potash salts in the soil, the addition of the nitrates of soda or lime leads to a double decomposition, and the conversion of the potash into nitrate. Sulphates and chlorides of these bases appear to have some small value as manure, although their composition remains unchanged; but in the mysterious laboratory of the growing plant the nitrate of potash is resolved into its elements. The potash allies itself with carbonic acid to form carbonate, or with carbon, oxygen, and hydrogen in various proportions to form the organates of potash (the citrates, tartrates, oxalates, etc.), so important to the development of fruits.

Whether we employ ammonia or the nitrates as manure, the nitrogen is liberated in the plant to unite with oxygen, and be radiated as common air. In the one case, hydrogen remains; in the other, potash.

The current theory of nitrogenous manure appears to be based on a complete misconception as to the function of the nitrogen in its various compounds; and when it is once clearly realized that hydrogen is the important food-substance yielded by ammonia, it will be of practical interest to determine whether this substance cannot be supplied more economically by the decomposition of water *secundem artem*.

C. F. AMERY.

Geological Questions.

THE replies to the following questions by some of the most eminent American geologists have induced me to ask your assistance in getting a wider circle to consider them. They were framed for the purpose of enabling the writer to properly represent American thought on the subjects mentioned, in his report on the Archaean to the American Committee in August next. Those geologists who are willing to render the undersigned the valuable assistance of expressing their opinions on the matters involved, are requested to write the letter of the question, and give the answer as laconically as is consistent with a clear statement of their views. In alternative questions, like J or N, it will suffice to append the numbers of the clauses representing their opinions.

A. Do you agree to the suggestions contained in the report of the International Committee on Nomenclature ('Report of the American Committee on the Work of the Geological Congress,' pp. 49 to B, p. 57)? Please state explicitly if you are willing to accept the recommendations of the congress.

B. Do you favor the division of the Archaean Group into a definite number of systems? If so, give their names and the order of their succession.

C. Give the horizons of non-conformability in the Archaean.

D. Do you approve of the plan of subdividing the Archaean petrographically and of omitting corresponding chronological divisions and names?

E. Should the eruptives occurring in the Archaean rocks be classified with the latter, or separately?

F. Which, if any, of the following terms is applicable in American geology, and how applied? 'Hebridean,' 'Dimetian,' 'Arvonian,' 'Pebidian.'

G. 'Are there crystalline rocks in, and after, the Paleozoic lithologically indistinguishable from those of the Archaean?

H. Are there any crystalline rocks in the Archaean which do not occur later?

I. Is mineral constitution indicative of geological age?

J. Are the lower stratified crystallines: (1) of aqueous origin metamorphosed partly, or wholly, by igneous action; (2) of igneous origin metamorphosed in part, or in whole, by subsequent agencies; or (3) partly one and partly the other?

K. Are there evidences of organic life in the Archaean; if so, where, and what?

L. In your opinion, is Eozoon Canadense of organic origin?

LL. Do you approve the European map committee's (Professor Lossen's) system of coloring and classifying the eruptives?

M. Should Serpentine constitute one class of eruptives?

N. Is Serpentine, (1) sometimes, or (2) always an alteration product: (3) of eruptives, (4) of sedimentary rocks, or (5) of either?

O. What, in your judgment, is the proper disposition of the term 'Taconic?' If employed, what are its limits, and what terms should it replace?

P. How should the Cambrian be divided?

Q. Are 'Menevian,' 'Ordivician,' or any other more or less comprehensive foreign names, applicable in American geology? if so, how?

PERSIFOR FRAZER,
Reporter for Archaean.

Philadelphia, 201 South Fifth St., July 9.

The Charleston Earthquake.

IN reply to Prof. Joseph Le Conte's valued criticism (*Science*, x. p. 22), I would say that it seems to me that the method for estimating the depth of an earthquake-focus proposed by Mr. Hayden and myself differs radically from that proposed by Mallet in the 'British Association Report' of 1858. His inference that the horizontal motion has a maximum value where the 'angle of emergence' is 54° 44′ could be true only of normal waves. It cannot be true of the transverse waves. He ignores the transverse waves entirely in his formula; and the omission, I maintain, is fatal to its applicability. He also ignores the vertical component of the normal wave, which at such an angle is much more energetic than the horizontal component. What proportion of the horizontal motion is due to the normal waves can generally be determined at considerable distances from the origin when the facts upon the ground are clearly manifested. But at the very localities where such a determination is necessary for the application of Mallet's method the difficulty is greatest. It is just here, too, that all the components, vertical and horizontal, normal and transverse, blend together with such effect that not one of them can be ignored without fatal error. We must consider their total effect. But these motions compounded represent the intensity, i.e., the amount of energy per unit-area of wave-front. Mallet's 'circle of greatest destructiveness' has no real existence. It is a purely mathematical abstraction obtained by postulating conditions which do not have any separate existence.

Since writing the above, I have recurred to Mallet's paper, and find the following: "It is certain that in all great earthquakes the real mischief and overthrow at places pretty far removed from above the centre of impulse are done by the blow from the normal wave, which appears to come first; hence, the main observable effects are those of the normal, and we are justified and enabled, *in such localities*, to neglect the transversal. But within a considerable circle of area, whose boundary is evanescent, and whose centre lies at the point right above the origin, the actual effects of the transversal wave are very formidable, and can never be neglected." [Then why should he have suggested doing so?] "The ground beneath an object so situated, such as a house or pillar (as the distance from the origin to the surface is the minimum range of emergence, or shortest possible, and its energy therefore the greatest), is almost at the same instant thrown nearly vertically upwards by the normal wave, and at the same moment rapidly forced forwards and backwards in two directions orthogonal to each other; and this combined movement, which is that called 'vorticoso' by the Italians and Spanish Mexicans, is one that nothing, however solid and substantial in masonry, etc., can long withstand."

It is certainly a pleasure to find Mr. Mallet reasoning so justly; but in the remarks quoted it is apparent that he is taking account of

all the components of motion; which must give us the true intensity in just the sense that this term is employed by Mr. Hayden and myself. Its graphic representation will be the curves we have given and no other.

Professor Le Conte remarks: "We have assumed all along that the intensity or excursion of the earth-particle, or the height or amplitude of the wave, varies inversely as the square of the radius of the agitated sphere. The authors as well as other writers assume this law." Here he evidently misapprehends. It is indeed assumed that the intensity varies inversely as the square of the distance, but the amplitude varies (subject to later qualification) in a simple (not duplicate) inverse ratio with the distance. The intensity for a given wave-length is proportional to the square of the amplitude; for, by Hooke's law (*ut tensio sic vis*), the time of vibration of a particle in an elastic wave of given wave-length is uniform whatever the amplitude. Hence the mean velocity of the particle is simply proportional to the length of its path, i.e., to the amplitude. But its energy is proportional to the square of its velocity, ergo, to the square of its amplitude. Hence, too, the amplitude must be inversely proportional to the radius of the spherical wave, provided no energy is dissipated in transmission. If, then, the amplitude at Charleston were four inches, at a distance of a thousand miles it would, without dissipation, amount to about two millimetres, — a well-marked tremor.

Professor Le Conte's suggestion that the law of variation of intensity with distance may be affected by reflection back into the earth from the surface is, so far as I am aware, a novel one. That there must be some energy so reflected seems undisputable. But the portion so reflected would constitute a new wave, or series of new waves, independent of those already in progress. It would thus add to the number of waves without affecting the energy of those already in progress, except at points of coincidence or interference.

Seebach's method of finding the depth was objected to, because it requires a degree of accuracy much beyond the highest we can hope to attain. The speed of an earthquake-wave is enormous (the time-observations obtained for the Sonora earthquake give a very high wave-speed; they are not as yet fully examined and discussed, but the preliminary examination indicates a speed about the same as that obtained in the Charleston earthquake), the space-intervals at which the time-records must be made must be short, and the time-intervals correspondingly so. The data really needed are differences in these time-intervals; and these differences would most certainly be much smaller than the probable errors of observation.

C. E. DUTTON.

Washington, July 9.

The Freezing-Point of Sea-Water, and the Melting-Point of Sea-Water Ice.

THE difference existing between the result from my determination of the freezing-point of sea-water (*Science*, ix. No. 228), and the accepted one as value for the same of 28°.8 F., seems to be inexplicable, unless we can assume that in the methods followed for its determination a wrong interpretation has been put on one of the results.

There can be no doubt, that, if the temperature of a body of sea-water is lowered till conglaciation takes places under slight agitation of the water, the temperature then existing at its surface will be that of its freezing-point.

On the other hand, it seems probable, that, when the determination of the freezing-point is made by means of an admixture of sea-water and its ice in thermic equilibrium, we have reached a condition that would be better described as the melting-point of sea-water ice.

Could we assume that in the change from the liquid to the solid form, in freezing, all the saline particles were taken up without chemical changes, it would be reasonable to suppose that the melting and freezing points would coincide; but if, on the other hand, we assume that in this conversion the entire saline particles have been expelled from the solid, we must conclude that part of the heat was expended in expelling these particles, for we may not imagine *any* work performed without a corresponding absorption of energy. We will have, in this imaginary case, essentially fresh-water ice; and, if we were determining the freezing-point of sea-water with ice so constituted, thermic equilibrium would be obtained at a temperature of 32°, which we should erroneously call the freezing-point of sea-water.

Granting the accuracy of these two suppositions, it seems certain that in the case when freezing takes place to the exclusion of four-fifths of the saline particles, as is the case with sea-water, thermic equilibrium will exist between sea-water and its ice at a temperature intermediate between its freezing-point (26°.7 F.) and that of melting ice (32°.0 F.), and experiment has proved this temperature to be 28°.8 F.

I would therefore predict, that, in the case where a liquid is converted into a solid by freezing, the temperature of the freezing-point of the liquid will be equal to that of the melting-point of the ice, only in the case or cases where each contains the constituents of the other in the same proportion.

W. A. ASHE.

The Quebec Observatory, July 4.

Concerning Filth-Diseases.

THROUGH the heart of the city of Baltimore, flowing southward, runs the bed of the sluggish stream Jones Falls. Eight miles northward, its waters are divided and turned into the city water-supply. Within the city, the stream is confined by handsome stone walls, which form a canal of dimensions twenty feet deep by a hundred feet wide and two and a half miles long — roughly. The canal empties into the Back Basin, a nearly stagnant pool two hundred yards wide by five hundred yards long, — which itself is connected by a short canal with the City Basin and tide water of slight activity.

In the northern suburbs, and within the city, the Falls and Back Basin receive the drainage from a territory in which dwell eighty thousand souls, roughly estimated, a considerably portion of whom is packed into a lower quarter of the city. They receive that from the Causeway and a part of Fell's Point, — quarters fairly designated slums.

The sediments in this drainage are precipitated in the lower half-mile of the Falls and in the Back Basin. Here they undergo fermentation and decay, at times giving off odors offensive indeed. It is a necessity of the situation that these sediments must be removed by dredging; and with the active officials of the dredging companies, and with their workmen, the writer has been in quite constant communication for nearly three years. These people pass their days stirring about and digging up this fermenting and decaying city garbage and mud and sewage. They live in an atmosphere loaded with offensive gases. And what of their health? With singular unanimity they declare that the occupation is a healthy one. Excepting in rare instances a case of nausea and vomiting, which quickly pass away, they have no more sickness than those in other occupations. As a matter of fact, the writer has not in nearly three years heard of any case of zymotic disease among about a hundred men engaged in this dredging.

The decaying refuse from the slums of a city, deposited in warm and nearly stagnant waters, ought to contain all manner of poisonous elements, — animal, vegetable, gaseous, or otherwise. Men stirring up and removing such material ought to sicken and die. Curiously enough, they do not — more than men in other occupations.

The writer has no knowledge as to what filth-diseases are, or are not, and has no suggestions to offer. The studies here indicated were made because the field seemed to promise a rich harvest of such diseases, but the promise has not been fulfilled so far.

WM. GLENN.

Baltimore, July 11.

Queries.

8. WHOOPING-COUGH IN THE CAT. — A Liverpool cat is reported to have contracted whooping-cough from a boy sick with that disease. For two weeks it had five or six attacks daily of the cough characteristic of that affection. Is this unusual? X.

9. BANANA, COCOANUT, AND INDIA-RUBBER. — Can any one send us lists of books on the cultivation of the banana, cocoanut, and india-rubber? J. C. E.